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THE PRESENT STATUS OF THE PROBLEM OF THE RELATION BETWEEN MIND AND BODY[1]

I AM not sure that the title of this paper conveys the proper idea of its contents. It might have been more correct to call it "The Ghost Theory of Animal Behavior." But that might have impressed the hearer as too sensational.

At the last meeting of the Psychological Association, during the discussion of the place of psychology in medical education, one of the speakers found it necessary to warn against including in the teaching of psychology a discussion of the problem of the relation of mind and body. Let me say at once that I hold, on the contrary, that the problem of the relation of mind and body is the chief one, if not the only one, for the adequate discussion of which the medical student should turn to psychology, for practically every other content of the modern science of psychology is available to him in his courses other than those going under the name of psychology. Yet in spite of the apparent contrast of opinion, as just stated, I feel certain that my own ideals are not essentially different from those of the gentleman referred to. The contrast of opinion results chiefly from the meaning of the phrase "relation of mind and body." I object to throwing the problem of the relation of mind and body out of the curriculum of a medical student just because some teachers of psychology can see in it no more than the endless repetition of traditional metaphysical speculation.

If we follow the traditions of past centuries, a discussion of the relation of mind and body is merely the discussion of metaphysical arguments in favor of adopting the one or the other of the two metaphysical war-cries, interaction or parallelism. I can readily understand why any one who expects of science, not terms suitable for shouting, but terms suitable for clearer and more comprehensive thinking, should get disgusted with these terms interaction and paral-

[1] Read before the Western Philosophical Association, University of Chicago, April 6, 1912.

lelism. If these terms mean anything to me, they mean this. Any mental state of mine, I am convinced on scientific, empirical grounds, is in a specially direct manner dependent on (mathematically speaking, is a function of) one or more variables of the nature of nervous activity. Suppose we refer to such mental and nervous variables as corresponding values. Then the question arises: Do such corresponding values make their appearance in our experience strictly *simultaneously* or *in succession*? In the latter case we have the relation of cause and effect; that is, we accept interaction. If, however, there is strict simultaneity, we can not speak of the relation of cause and effect; that is, we accept parallelism. Now, it is almost incomprehensible that philosophers should have wasted their energies for centuries in order to derive from metaphysical arguments an answer to a question which can be answered only by appealing to observation. Imagine that geographers had attempted to derive from metaphysical speculation an answer to the question whether the North Pole was located on the ocean or on a continent. They had to wait patiently until some one had made the observation. We shall have to wait patiently until an instrument (let us think of an X-ray mirror) will have been invented which enables a person having a mental state to observe the corresponding value, the corresponding objective process in his own nervous system without the slightest interference with the normal function of this nervous system. Then we shall be able to decide whether the corresponding experiences, subjective and objective, are strictly simultaneous or successive. Until then let us wait and not spend any more time on interaction and parallelism than what is sufficient to describe the problem to the student as a problem whose solution lies in the future.

With the rise of modern biological science parallelism seemed to be destined to beat its rival into oblivion. But a curious reaction has set in, and the latest book on this subject-matter, that of William McDougall ("Body and Mind") steps before the public eye as an outspoken and very able defender of interaction. What has brought about this evolution and attempt at revolution, for no name other than revolution seems to me significant enough for the attempt to answer our question thus one-sidedly? It might be said that the preceding parallelism was equally one-sided. As a matter of fact, this can not be said of the parallelism of that class of men whom we may compare with McDougall—of the biologists. With the biologists, especially those of the nineteenth century, the confession of parallelism did not mean the dogmatic solution of the problem which, as just stated, can be solved only by future observation; it really meant only a confession of their belief that animal life, including human life, in all its phases and without any exception, could be

scientifically described without any reference whatsoever to subjective states, to states of consciousness. McDougall sees that this is the meaning of the term parallelism in the biology of the last centuries. He sees this so clearly that he defines his own view, interaction, as meaning that a scientific understanding of animal life is made possible only by introducing into the chain of causes and effects the subjective factor, consciousness.

Should we side with the majority of the biologists or with McDougall and those others of a similar trend who assure us of the insufficiency of a *purely objective* science of animal life? Before I give my answer, I invite you to glance back over a few thousand years of human thought. There was a time when no strange event seemed comprehensible to the human race unless it was referred to a god, a ghost, a demon as its source. Lightning was fire thrown by the weather god. A king eating grass did so because he was possessed of a demon. A friend found dead in his bed in the morning had been smitten by a ghost. We no longer think in this way. We no longer think of an epidemic, for example, as the work of gods taking revenge. Is not all progress of modern science due to the fact that scientists have consistently discarded all *ghosts* as causes explaining any natural phenomenon? And now we are asked to be inconsistent. In the explanation of animal, and especially human, life we are asked to introduce the ghost, consciousness, as a cause. Truly, before we take such an inconsistent step, strong proofs should be required that thereby we may hope to gain a scientific advantage.

Let no one object that introducing consciousness into the explanation of animal life, of animal behavior, is not the same as introducing a ghost into the explanation of an epidemic, for one's own consciousness is surely not an illusion. But here is the point: "one's own." The scientist who gives an explanatory description of an epidemic does not describe the disease from which he is suffering himself, lying on his death bed. He describes, if not exclusively, at least chiefly, his experience of the diseases which have stricken other people. And the scientist who portrays animal behavior describes chiefly his experience of the behavior of other organisms, not his own. But the *consciousness* of other organisms is not an experience. It is a ghost introduced for the purpose of explanation like the ghost introduced for the purpose of explaining an epidemic.

What, now, are the scientific advantages which are offered us, if we show ourselves willing to deviate from the established custom of several centuries of scientific progress, if we show ourselves willing to introduce, in our study of animal behavior, the ghost into our explanations? Let me quote McDougall, whom I regard as the ablest champion of the "ghost theory" of animal behavior. He adopts for

this theory the name of "animism." He says: "Animism recommends itself because it points to a great unknown in which great discoveries still await the intrepid explorer, a vast region at whose mysteries we can hardly guess, but to which we can look forward with wonder and awe, and towards which we may go in a spirit of joyful adventure, confident in the knowledge that, though superstition is old, science is still young." I have been unable to find that McDougall claims for his view any other scientific advantage. I do not know how others estimate the weight of this one; to me it has no weight at all. I am too much aware of the present incompleteness of our neurological science, of the existence of a great unknown lying there before the intrepid explorer, too enthusiastic and hopeful in my endeavor to clear up my notions of the manner in which animal behavior depends on nervous functions, too much embued with the spirit of joyful adventure in the field of objective science,—to turn to the ghost theory for a mental tonic, for inspiration and encouragement. If that is the whole scientific advantage offered by the ghost theory, I must say that I do not need it. Let him accept the ghost theory who has already despaired of further progress of the objective science of nature, who needs a bracing up. I do not need it.

If there is no real scientific advantage attaching to the ghost theory, how are we to understand its reawakening, under the name of interactionism or animism, among psychologists, after it had seemed, for many years, to have been laid into the grave with its last defender, Lotze? There are two reasons for this. First, it had suffered under an argument unjustly wielded against it. The assertion had been made and had been generally accepted that the theory was incompatible with the law of the conservation of energy. Fifteen years ago Stumpf succeeded in pricking this bubble. Unfortunately, however, some psychologists mistook the annihilation of that hostile argument for a positive proof of the value of the ghost theory, which, obviously, it could not be. The second reason is of greater significance. The neuron theory held its sway over neurology, and, as a part of this theory, appeared the doctrine of the synapse. The ear, say, is stimulated. A nervous process runs along a neuron, but only to find itself blocked at a point which is both an end point of the path thus far taken and a division point from which many directions may be taken. The tension becomes greater and greater. The protoplasm stretches out its arms like an amœba and touches the protoplasm of another neuron. The nervous process then crosses this bridge. Thus far this seems plausible, and the doctrine of the synapse has always seemed plausible to the neurologist who asked no further question. But the psychologist asks a further and abso-

lutely essential question: Why does the protoplasm stretch towards one neighboring neuron when the organism happens to be in one situation, towards another neuron when the organism is in another situation? General silence with the neurologists. But some psychologists had an answer ready. They brought in their *deus ex machina.* The ghost does it. Consciousness, feeling, will, or whatever you call it, turns the bridge in the proper direction as the switchman turns the switch in the railway yard. Thus the doctrine of the synapse is largely responsible for the reawakening of the ghost theory of animal behavior.

Although it does not interest us directly, I can not forbear mentioning as a curiosity the fine reasoning of some psychologists telling us that the mere causal determination, selection, of one direction among many thinkable ones did not require an expenditure of energy and therefore could well be regarded as the work of the ghost. As if the direction of anything, say, a pole losing its balance on the tip of the nose of a circus clown, could be causally determined without the expenditure of energy.

It was among European psychologists chiefly that the physiological doctrine of the synapse reintroduced the ghost into the explanation of animal behavior. In America the ghost became popular through the great influence of one man, James, whose followers assign to one kind of mental states which does not seem to have any proper business, to the feelings, the job of stamping in and stamping out complete paths of nervous conduction. But they never state any definite law explaining how the proper feeling itself, with its stamping power turned in the proper *direction,* comes into existence at the proper time.

We now reach the crucial point of the issue which I intend to present. According to McDougall, those who reject, or do not favor, the ghost theory of animal behavior do so because they lack the courage to accept an incomplete world picture. But I charge that, on the contrary, those who adopt the ghost theory lack the courage to accept an incomplete world picture and to wait for future research in natural science to complete it. Too impatient to wait, they fill in the gap with a ghost, with unexperienced consciousness, with the concept of something which is unmeasurable, to which none of the methods of scientific research are applicable. If any one claims that my assertion is wrong, that the methods of scientific research are applicable to the ghost which is made to bridge the gap of causal connections in animal behavior, I challenge him to state a single instance. He will not assert that such work as the classical experiments of Ebbinghaus on memory serves as such an instance. McDougall himself admits expressly that they are a purely objective

study of verbal habits, that they are no measurements of consciousness.

How, then, did men, so much in earnest about psychological progress as McDougall, become so overwhelmed with despair that they had to appeal to the ghost theory for help, or rather for mere comfort? The answer is simple. They attempted in vain to conceive of a nervous process as being capable of forcing another nervous process from its own path into a new path. It is the demand for such a conception that I have tried to supply in my book on the "Fundamental Laws of Human Behavior."

The most important concept applied to animal behavior is that of *an experience.* We mean by an experience that an animal, in a new situation, acts in a new way in response to the same stimulus, that is, that the nervous process from a certain sensory point does not pass along the path of least resistance, but along a path of higher resistance. In order to understand that a nervous process proceeds over a path other than that of least resistance, we must speak of its being *forced.* But we need not speak of its being forced by a ghost. When a *person,* say, a school-boy, instead of moving along the path of least resistance, which leads to a circus parade, is forced away from this path towards his school, he is forced most probably by *another person,* his mother, or a truant officer; but certainly not by a ghost, a good or evil demon. When a *nervous process* is forced to stream over a path other than that of least resistance, it is forced most probably by *another nervous process.* If psychologists had been less slow in thinking this simple thought, they would have been less quick in introducing the ghost who is supposed, in the nervous system, to take a place equivalent to that of a switchman of a railway yard, or a lineman of a telephone company, or a stamper of a sheet metal factory, but who, in the nervous system, is simply a *deus ex machina.* I have shown in my book that it is possible to understand all the fundamental facts of animal life experience by simply conceiving of any nervous process as capable of forcing, under certain conditions, any other nervous process out of the path of least resistance into another definite path. The doctrine of the synapse is then entirely superfluous. To enter into the details of this conception and its application to the various forms of animal (including human) behavior, this is neither the time nor the place.

If, then, a purely objective science of animal behavior must be the ideal towards which to strive to-day as much as, and even more than, at any previous period of science, can we afford to omit all reference to subjective states in the instruction given to scientific, and especially medical, students? May be that the time will come when we can afford it, but my study of the most modern advances in

that branch of medical science which particularly concerns us, in psychiatry, furnishes me sufficient proof that that time has not arrived yet. I have in mind that successful movement of studying and treating hysteria and related disturbances which has become associated with the name of the Austrian psychiater, Freud. His analysis of the individual's life leads to a systematic reeducation of the organism along definite lines, which is beginning to replace the former therapeutic methods of strong, but haphazard suggestions, hypnotic or non-hypnotic; and this analysis is made almost exclusively in *subjective* terms. It is too early, then, to renounce under any and all conditions all subjective terms in psychology. We can not put them out of the world by putting, like the proverbial ostrich, our heads in the sand so that we do not see them.

But then, certainly, it is impossible to keep the purely objective description of animal, or let me now rather say "human," behavior and its subjective description in tight compartments; but a mixing up of them is equally unjustified. We need to establish definite relations between our subjective and our objective terms, so that, instead of mixing them up, we can translate the one into the other. Then only will it be possible to utilize the advances made at the present time in psychiatry for the advancement of an objective science of human behavior. We must try to establish *definite nervous correlates for all the specific mental states and mental functions* which are used in and seemingly can not be spared from our descriptions of human life in the mental and social sciences. I venture to predict that those terms of mental function, for which no nervous correlate can be found, are the very ones which are superfluous, can be spared from our descriptions of mental life in man and animals. When a few years ago I made an attempt at establishing some such nervous correlates, I found to my surprise that most psychologists did not seem to see the use of them. They failed to see the difference between such definite correlates and the vague generalities of our text-books stating that, whenever anything is to go on in our minds, something must go on in our brain; or that, whenever any brain function is fixed, it is fixed by the satisfaction which it gives to the mind. Such generalities may be true; but to me it makes no practical difference whether they are true or not, because they are no solutions of scientific problems, for reasons stated throughout the whole length of this paper.

In the establishment of definite correlates of specific mental functions and of specific nervous functions I see the present-day problem of the relation of mind and body.

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DISCUSSION

SOME ASPECTS OF PROFESSOR FITE'S INDIVIDUALISM

I FIND myself with considerable sympathy for what I interpret as Professor Fite's purpose in his recent book. Indeed I am not sure that what I have to say involves much more than a change of emphasis. I am ready at any rate to agree that the logic of a fully conscious individualism looks in the direction which he urges. The principle of democracy, as distinguished from what may loosely be called communism, is indeed just this, that each man shall, not surrender his aims to the general welfare, but adjust them to a full and free recognition of the similar aims of other men, on the faith that only thus can he fulfill his own life most abundantly.

But along with a communistic ideal of the state, such as Professor Fite seems to have chiefly in mind to criticize, motivated by altruistic feelings, and logically dependent, therefore, upon the somewhat remote hope that men can be induced voluntarily to surrender such advantages as they possess to their less successful neighbors, there is an alternative position which, though sharply opposed to the conception of democracy, adopts equally with it the presuppositions of individualism. It differs, however, in giving to certain individuals a preference, and in holding that their more important claims can only be met through the absence of a complete autonomy and satisfaction in a considerable number of their fellow men. Of course no one who is not entirely stupid can fail to see that the logic of his own private interest demands that he allow *some* other men to get their way, too. But plenty of people do believe, with much confidence, that they can and ought to stop short of a universal tolerance.

Now at this point I have not been able to make up my mind with certainty just what Professor Fite's attitude is. On the practical side I suppose he intends at least to say this: first, that people can never be largely benefited until they have an intelligent understanding of their own needs and purposes and are ready to assert these for themselves, instead of leaving them to the good will of others; and, secondly, that schemes of social reform, to be effective, must be framed primarily to appeal to interests, rather than to benevolence and charity, to supply their motive force. So far I am inclined very largely to agree, as a question of where the emphasis had better lie in the promoting of political and social measures. Talk about humanity and disinterested justice has indeed an important preparatory value in breaking up the inertia of the public mind in the face of new proposals, to which I doubt if Professor Fite is altogether fair. But after all if concrete changes are to be brought about and

are to continue to work well in practise, men have got to be shown that these are to their interest. The average citizen, for example, must be made to realize that his taxes are increased or his business opportunities lessened by public graft, before he can be held in line for municipal reform; and the disposition to substitute such definite economic considerations that come home to self-interest, for humanitarian exhortation, is one of the best guarantees of the probable success of any wave of reform. Back of this there may be, and indeed I have no doubt there must somewhere be, a temper of moral fervor. But the less we talk about this and accentuate it as the professed motive, and the more we apply ourselves to the rational business of working out the situation in a form to enlist a sufficient multitude of private interests, the more reforms are likely to lose their spasmodic character and become settled principles of action.

But now while this is good advice to the reformer and to those in whose special interest a change is sought, I do not feel so clear about the state of mind which it recommends to the powerful classes who are already in possession, or who by their superior intelligence have the immediate directing of the future. So far as bringing influence to bear upon them goes, I agree, because it seems to be the fact that we are foolish to trust much to exhortation. We ought rather to gird up our loins and convince them that they can not disregard us with safety to themselves; and in so far as they are intelligent they will doubtless in the end see the point and act accordingly. But what is the temper of mind that Professor Fite would ethically approve and justify on their own part? Does reason prescribe that they wait passively for the corresponding development of intelligence in other men, exploiting them meanwhile as without rights until they are able to enforce these rights? or does justice demand that they take such men into account from the start as potentially capable of autonomy, and so, as having rights to be respected? Professor Fite gives some ground for believing that the first is his meaning; if so, I have no wish to defend him. But his idealistic logic seems to me rather to look the other way. Much of this appears without point unless it intends to hold that a complete self-interest will find itself imperfectly fulfilled, except as others are equally self-conscious and autonomous; and if this is so, one is failing in duty to himself unless he does what he can to further the development of security for equal rights to all, even before these can be enforced upon him. The same claims would thus rest upon him as on the ordinary showing; only the *source* of these would be his own welfare, rather than something from the outside that calls for sacrifice and altruism. Subject to correction, I am inclined to suppose that this is really Professor Fite's meaning, and that apparent evidence to the contrary is due to the

fact that he has not sufficiently separated two different standpoints—the standpoint of the reformer, who asks what he can safely presuppose in other men as a working basis of reform, and the inner standpoint of the intelligent man himself in face of the question what rights he shall concede voluntarily to his weaker neighbor.

But here another query arises about Professor Fite's philosophy. He has, as I understand him, a twofold problem. Primarily, perhaps, he is trying to refute what he considers the sentimentalism of the humanitarian. But also he is attempting to justify rationally the claims of social conduct apart from such an altruistic motive. Now it is when the aristocratically-minded man is to be convinced of this that I feel a lack of conclusiveness on Professor Fite's argument. I agree that the most likely way to reach him is by showing him that he is playing the fool, is ignoring facts which he ought to face, and which are preventing the best attainment of his own desires. But I hesitate to believe that this demand is always capable of being met completely, or that it is sufficiently met by an appeal to the nature of consciousness as such. And the reason, on the side of theory, is this, that I find it difficult to separate intelligence from the particular nature of the desires which it may endeavor to serve. The inclusiveness with which a man is going to admit foreign ends within his own system will depend upon the character of the objects which he thinks worth while attaining; and this can not be assumed forthwith as of just one standard quality. What am I to say, for example, if I come across an ideal which apparently gets satisfaction through compelling as many other men as possible to do its bidding—which seems to aim at the very act of keeping others under, because this affords an enjoyable sense of superiority and power? The only thing that can be counted on with certainty is that a perfect intelligence will aim to take account of all the facts, but not that it will necessarily accept as among these facts the legitimacy of another person's ends. It is conceivable that as much intelligence may be shown in recognizing such a competing end and then finding ways to override it, as in accepting it and adjusting action to its requirements.

And to this there are only two answers that I see. It may be said that you are losing something, after all, from the content of the world when you exclude the contribution which another man might bring if he were permitted to follow his own bent. From the world, perhaps, but why of necessity from *my* world, unless I happen to be built so that I want it more than I want its exclusion? His economic contribution I may easily be indifferent to, even if it were clearer than it is that some of it would flow to me. There may be a chance that he may put some obstacles in my own path, but possibly I enjoy the excitement of combat and exploitation. If it is claimed, again,

that by admitting him into my circle I realize the finer spiritual joys of cooperative fellowship, this may very well be so in case I find myself caring for an enlargement of this sort, but not at all if I happen to have an aristocratic taste for power. It will not do to say that no man actually does prefer this last ideal, and that his nature will in reality get a fuller expression in the other way. I may hold this as a faith; but I can not demonstrate it while so many are convinced to the contrary. And in any case the ground for my faith will be, not the abstract character of consciousness, but the concrete nature of the being who is to make use of intelligence to further his ends, those ends being set by his inborn make-up and natural disposition, which apparently differs, within limits, in different people. If a man has seemingly other wants than mine, which look to empire rather than cooperation, I can not refute him by pointing out that intelligence—and he of course wishes to be intelligent—is never complete until it has thoroughly grasped the standpoint of every would-be competitor. He will answer that he intends to understand them; but as for sympathizing with them and accepting their claims, that is another matter. To do this may be precisely to defeat his own particular aim. To enter into their hopes with toleration and sympathy would require that he be another sort of being from what he is—that he be of a nature to suffer directly some diminution of his own sense of attainment through an outlying loss to another man. Assume a satisfaction in fellowship independent of the special character of the task to which cooperation is turned, or an intrinsic disinclination to view with indifference a loss to others over and above the indirect effects that this may have on my own enterprises, and you may indeed expect results. As a matter of fact I suspect that Professor Fite does assume this, and that to it his argument owes the generous quality that might have been quite lacking. But this looks suspiciously like bringing back again the notion of a disinterested side to human character on which the effective appeal of motives to social conduct depends, and the proof of this carries us beyond the abstract logic of self-consciousness.

Accordingly, while I agree that ordinarily the best way of proving to any one that he ought to regard the rights of others is by showing him that he is acting unintelligently otherwise, I should expect to be able to do this, not by a deductive argument from the nature of consciousness, but rather in an empirical way, by calling his attention to the actual nature of the world in which he lives, and the circumstances of the case. But then I should have to give up the hope of convincing him that the harmony was bound to be a complete one. I should be content if he were persuaded only that this was the *better* way, though not of necessity a way which involved no elements what-

ever of loss to him. I might fall back on the faith that apparent loss will after all prove real gain. But so long as knowledge confessedly is incomplete, this would have to be faith, and not philosophic insight. Even if I came up against an ultimate difference of ideal I should not despair of finding solid reasons for my own side. But in that case, at any rate, I should have to admit a solution which was of the nature of a compromise, which came about at the expense, to some degree, of a real preference, and was, therefore, a reconciliation only partially complete.

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SOCIETIES

NEW YORK BRANCH OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

THE New York Branch of the American Psychological Association held its final meeting for the current academic year on May 22, in conjunction with the Section of Psychology and Anthropology of the New York Academy of Sciences. An afternoon session was held at the Psychological Laboratory of Columbia University. After dinner at the Faculty Club the evening session was held at the American Museum of Natural History. The following abstracts are of the papers presented at the two sessions:

Group Differences in the Interests of Children: GERTRUDE MARY KUPER.

That interest plays a very important dynamic rôle in the educational field is only too evident from such treatises as Dr. Dewey's article, "Interest as Related to Will" and Dr. Montessori's "Pedagogia Scientifica." But interest is a general term and can not have an absolutely universal value for every individual or every subject of thought or desire. Individual interests are as important in the social world as are individual capacities. They should, therefore, be a fruitful field for scientific investigation. The experimental work done with advertisements has brought to light group differences in the preferences of men and women for various appeals. The investigation to be reported was of a like nature, except that it dealt with children.

The formal experiment consisted in asking an individual child to arrange nine pictures in the order in which he liked them best. The nine pictures were chosen to represent nine specific appeals: landscape,

children, animals, religion, pathos, sentiment, patriotism, heroism, and action. (They were Cosmos prints and therefore of uniform size and finish.) In all, there were three series of these pictures, each parallel so far as possible with the other two in their appeals. The children numbered over 200, 10 girls and 10 boys for each year's age from 6.5 to 16.5. They were almost entirely attendants of the public schools of New York City and came from quite varied sections of the city.

The results were tabulated according to age differences, broad social distinctions, and nationality. In the last-named case the number of subjects was so limited (10 girls and 10 boys to each of the following nationalities: Irish, French, German, and Italian, and only 9 girls and 8 boys to the Spanish) that the results are not held as significant.

The positive data showed a sex difference in the order of preference for these several appeals. The girls' order was: (1) Religion, (2) patriotism, (3) children, (4) pathos, (5) animals, (6) sentiment, (7) landscape, (8) the heroic, (9) action. The last two were decidedly lowest in the scale and the first three were quite clearly highest for all ages; but the picture representing these nine curves was one of bewildering intersections as the values changed from year to year. The boys' order was: (1) Religion, (2) patriotism, (3) action, (4) the heroic, (5) pathos, (6) animals, (7) sentiment, (8) landscape, (9) children. The boys' chart representing the curves for these appeals showed greater agreement from year to year. Religion and patriotism, the heroic and action, and landscape and children kept rather parallel courses all along the age scale, and no very decided tendencies appeared with progressive age differences. Girls seemed to lose interest somewhat in pictures of children and animals and to take greater interest in the heroic and action pictures. The latter change is explained by the fact that, as the girls increased in school knowledge, they read an historical background into these more or less warlike scenes.

A great sex difference was found in the variability measures, as calculated for the various ages, appeals, social classes, and nationalities. In every case but two, the girls exceeded the boys in their P.E.; and in these two exceptions the boys' P.E. was once greater than the girls' by only 5 per cent., and another time exactly equal to the girls' P.E. The amount of sex difference was, as a rule, anywhere between 12 per cent. and 57 per cent. This held true in every scale, whether according to age, appeals, social class, or nationality. The girls' average P.E. was 1.66; that for the boys was 1.36.

Both girls and boys were least variable about the subjects they

liked best, *i. e.*, religion and patriotism; but apart from these appeals there was no correlation of variability with relative likes or dislikes.

It is a noteworthy fact that in range of variability the boys far exceeded the girls. The limits for the boys' P.E. were .82 (patriotism) and 1.60 (landscape), giving a range of difference of 78 per cent.; the limits for the girls' were 1.47 (religion) and 1.95 (animals), showing a range of only 48 per cent. In this particular experiment this indicates that boys are very much more agreed about some likes than are girls, and yet quite as varied about others. In other experiments such a range of variability may point to greater individuality of the male sex among themselves while as a group they are relatively homogeneous.

Another sex difference noted was the number of positive dislikes expressed by each sex. The girls gave 161, or 6 per cent., dislikes as against the boys' 65, or 2.4 per cent. Boys seemed to entertain relative indifference toward the appeals at the bottom of the list. The things the girls disliked most were, (1) scenes of action suggesting death and (2) pictures showing angry attitudes. The reasons given by the boys for their dislikes were, (1) gloomy, indistinct scenes, (2) sentimental pictures, (3) costumes worn by men which were feminine in style or left the figure partly nude, and (4) pictures suggesting illness.

A certain age difference revealed itself in the remarks made by the children about the pictures. The seven and eight year olds showed limited powers of observation. Some detail, and, in landscape scenes, always the human detail, no matter how small, was made the focus of attention to the complete overlooking of the larger subject. Unfamiliar details when pointed out to them received as many different interpretations as there were children. As the children grew older their remarks were fuller; they made fewer mistakes in their interpretation of the pictures and they drew upon all their known sources for filling in their perceptions. At the ages between 11 and 13 the critical spirit made its first appearance among the girls. Only at fourteen did it occur in the boys' comments. At these ages the emotions prompted the remarks of both girls and boys. Emotional attitudes, actions, and even words were ascribed to the pictorial persons. At 15, the remarks became more laconic, but what was said was significant and definite as to the persons, place, and action of the picture. This age marked the first signs of hesitation in speaking of the pictures of sentiment. Up to the age of nine the remarks had been very naïve; after that the pictures were dismissed with the phrase, "they're lovers" or "a love picture"; often the characters were named Romeo and Juliet, Paul and Virginia, etc.

In all their comments the girls were far more personal than the

boys. The personal pronoun and references to their individual experiences were the usual preface to their statements. With the boys it was quite otherwise; they discussed the picture as an objective thing, independent of their conscious existence. Boys tended to locate scenes in definite historical time and specific geographical places.

The effect of uncertainty about a picture, crudely averaged, was a displacement of about five places toward the lower end of the scale.

Practise in the Case of Children of School Age: THOMAS J. KIRBY.

This experiment was conducted to get some information concerning (1) the value of the practise experiment as a method for school work and (2) the value of practise periods of different lengths.

339 fourth year children belonging to 10 different classes took part in the practise, which consisted of adding columns, each of 10 numbers, 0's and 1's not included, as rapidly as was consistent with accuracy, each child competing with his own past record. Seven different sheets of columns of equal difficulty were used. (Thorndike's Addition Sheets.)

In every case there was one hour of practise, but for different classes this hour was broken into 22½-, 15-, and 6-minute periods, an initial 15-minute period and a final 15-minute period being given to form the basis for determining the gain per cent.

The hour's practise for the 339 children taken as one group resulted in an average gain of 55 per cent.; median gain of 48 per cent. In a similar test with 19 university students, Professor Thorndike found an average gain of 29 per cent., median 33 per cent., from about 53 minutes of practise, and said: "The amount of improvement in this experiment may also add to our confidence that the method of the practise experiments wherein one works at one's limit and competes with one's past record may well be made a regular feature in many school drills. Even if the same length of time produced in children a percentile improvement, only half as great as here, the gain would still probably be far greater than the gain by any of the customary forms of drill."

For the classes which took the hour's practise in 22½-minute periods, there was an average gain of 61 per cent., median 49 per cent.; in 15-minute periods, average gain 55 per cent., median 43 per cent.; in 6-minute periods, average gain 54 per cent., median 44 per cent.

The Age of Walking and Talking in Relation to General Intelligence: CYRUS D. MEAD.

I. *Data.*—50 "normal" children (25 boys and 25 girls), averaging less than six years of age, of graduate students of Teachers College and Columbia College. Ages were thrown to the nearest month.

Walking means: "To take a step unassisted." Talking means: "To use a word intelligently, *i. e.*, to associate the idea with the object."

Results.—The median "normal" child begins to walk at 13.5 months, with a probable error of 1.06 months. The chances are 999 to 1 that the true median will not differ from the median obtained by more than .66 month. The extreme range is from 11 to 30 months. 90 per cent. of the cases fall between 11 and 17 months. The median "normal" child begins to talk at 15.7 months, with a probable error of 2.83 months. The chances are 999 to 1 that the true median will not differ from the median obtained by more than 1.96 months. The extreme range is from 9 to 25 months. 90 per cent. of the cases fall between 10 and 21 months, with 18 months as the mode.

II. *Data.*—145 "schoolable" children (boys and girls) of the Indiana School for Feeble-minded Youth, in reply to the question on the personal descriptive entrance blanks: "At what age did the child commence to walk?" and 92 in reply to the question: "At what age did the child commence to talk?"

Results.—The median feeble-minded child begins to walk at 21.8 months, with a probable error of 7.56 months. The chances are 999 to 1 that the true median will not differ from the median obtained by more than 3 months. The extreme range is from 12 to 72 months. 90 per cent. of the cases fall between 13 and 50 months.

The median feeble-minded child begins to talk at 34.2 months, with a probable error of 12.6 months. The chances are 999 to 1 that the true median will not differ from the median obtained by more than 6.5 months. The extreme range is from 12 to 156 months (only one case going above 108 months). 90 per cent. of the cases fall between 14 and 84 months.

Sex Differences in Incidental Memory: G. C. MYERS.

A test was desired wherein the thing to be remembered should be merely incidental and where the focus of the subject's attention should be directed away from the facts to be called for after the exposure of the stimuli, but where these facts would have to enter, wholly or in part, into the experience of the subject. To this end a list of six simple words were used as stimuli. The subject was told that he would be given a spelling test and he was led to believe that it would be a real test in speed and accuracy of spelling.

A practise test with digits was given for three successive times before the real test began, to delude the subject as to the purpose of the experiment. A dozen or more digits were pronounced at random so rapidly that the subject could scarcely keep up in writing them. In the midst of this series of digits the experimenter, without

any warning, gave the signal for the subject to turn the page upon which he was writing, and continued to pronounce digits at the same speed. The subject was told that the words would be given in the same manner, but not quite so rapidly. The following words were then pronounced: angel, pickle, dirt, busy, onion, women. The last word was pronounced in such a manner that another word was expected by the subject, but the signal, "turn," was given instead, and the subject was told to write as many of these words as he could remember, to place them in the order in which they had been given, and to indicate by a line the place for each omitted word. The time each individual required to reproduce the words was recorded by a stop-watch.

After testing over 100 individuals the writer applied the test to groups of college, normal-school, and public-school subjects. Aside from immediate reproduction, records were secured after various intervals, ranging from ½ hour to 3 months. In all such cases a practise test of rapid folding of papers was added. After the words were pronounced the papers were promptly collected and the experimenter left the room. The subjects thought the work was ended, but at various times the experimenter reappeared and asked for the reproduction. The time for all group reproduction was limited to 1½ minutes.

The best results were secured immediately after presenting the stimuli. Practically the same efficiency was shown for the reproduction after 6 hours as for that after ½ hour. But there was a decided fall after 7 days and a still greater fall after 3 months.

No appreciable difference was shown in efficiency between the lower grades and the college students for immediate reproduction; but after various intervals there was a gradual decrease in efficiency with age.

Of the 1,515 subjects, 757 females and 758 males, only 29 of the former and 18 of the latter reproduced the six words in exact order.

In all grades the females were markedly superior to the males, both for the number of words remembered and for order. They had a higher central tendency and were more variable than the males in the 5th, 6th, 7th, and 8th grades, while for the other groups the males were more variable.

108 other subjects were tested with 10 letters and digits. Here the girls answered more, but the boys were better for order.

The Effect of Distribution of Practise Upon Learning: ELMER A. CULLER.

The purpose of this experiment was twofold: to determine the effect of differently distributed practise series upon learning given

material; and to make observations upon the learning process in general.

The material to be learnt was the path from the beginning to the end of the Hampton Court maze. The paper (8 by 6 inches) on which the maze was printed, was affixed to a board. Over it was placed a large circular piece of cardboard, easily movable, having in the center a small opening (⅝ to 11/16 inch) through which extended a pencil to mark the course of the subject's movement. At no time could the subject see more of the maze than the part visible through the opening. At the beginning of the experiment the subject was thus instructed: Pencil is now at the entrance to the maze; keep on moving until you reach the end. Never cross a line; always keep to an open path. Mazes are all the same and will be placed in the same position.

At each trial the time was recorded and number of errors was counted and recorded. To each subject were given 12 trials. Subjects were divided into 6 groups as follows: 12 trials at one time, 6 on 2 successive days, 4 on 3 days, 3 on 4 days, 2 on 6 days and 1 on 12 days. There were 5 men in each group except the last, in which were 3. With regard to time of day, subjects were divided into two groups: one group each day for the required number of days, after lunch (1–2 P.M): the second group each day after dinner (7–8 P.M). In comparing men of the two groups no account was taken of this slight difference, as it was considered practically negligible. Good light was uniformly provided. The interval between successive trials of a subject at the same sitting was 30–40 seconds.

Subjects were all graduate students, age from 22 to 28.

Three classes of errors appeared: Wrong choice between alternative courses, retracing when on right course, and (accidentally) crossing a line. The first kind are major errors (value 1) and the other two kinds minor (value ½). These are arbitrary values for computing results. The major errors were counted as follows: There are 6 (or 7, depending upon the course taken) places where choice must be made between alternative paths of which only one is right. Each time the subject moved from one of these places in a wrong path, *i. e.*, away from the goal, it was counted one error. Errors of retracing when on the right path were usually small and due to defective attention or eyesight—subject either thought he had accidentally passed an opening and moved back to see, or on coming to a turn failed to notice the opening and thought he had run into a blind alley.

The results are as follows:

I. TABLE OF ABSOLUTE TIME AND ERROR VALUES ATTAINED IN EACH GROUP (The different groups are indicated thus: One—12, etc.; the word indicates

the number of trials each day, the figure the number of successive days. The two columns show the average of number of seconds consumed and number of errors made in the last three trials in each group; thus showing the relative standing of groups at end of practise period. The figures in parentheses show relative position.)

	Time, Per Cent.	Errors, Per Cent.
One—12	50 (3)	4.8 (4)
Two—6	61 (5)	5.2 (5)
Three—4	59 (4)	3.2 (3)
Four—3	39 (1)	.9 (1)
Six—2	75 (6)	5.5 (6)
Twelve—1	48 (2)	3.0 (2)

II. Table of Percentage Gains

(In each case the percentage represents the ratio between the average of first three trials and last three trials in the same group. This table is intended to show improvement of each group irrespective of absolute values attained.)

	Time, Per Cent.	Errors, Per Cent.
One—12	210.0 (4)	147.9 (5)
Two—6	253.0 (3)	161.5 (4)
Three—4	195.0 (6)	302.0 (1)
Four—3	341.0 (2)	218.5 (3)
Six—2	206.6 (5)	125.3 (6)
Twelve—1	368.7 (1)	236.6 (2)

(It must be said that the results of Six—2 were vitiated by the professed indifference of one subject, because of which both time and errors for the last few trials in that group are abnormally high.)

The results seem to point to the following conclusions: In general, outside the Six—2 group, the One—12 and Two—6 groups made the lowest absolute records and also least improvement; this apparently indicates that the learning period was too prolonged, with insufficient practise at any one time. On the other hand, the Twelve—1 and Four—3 groups show in general the highest absolute records and greatest improvement. Here the practise was more thorough each time and not so prolonged. The curve of greatest regularity is the Four—3 curve. The three groups, then, in which practise periods were longer and confined to a few days show better results than the three in which practise periods are shorter and prolonged over 4–12 days. The application to learning any material would seem to be that better results are secured by a few more prolonged or persistent periods of study repeated perhaps for several days than shorter periods prolonged over a greater number of days.

Some observations were made on individual methods of learning which can not be included here.

Experiment in the Catching of Pennies: E. S. REYNOLDS, J. T. GYGER, L. L. WINSLOW.

The experiment had two aims: (1) To investigate the learning process. (2) To find what transfer, from the right hand to the left hand, if any, would be shown.

Three subjects took part in the experiment which follows. It was carried on in two series: (1) That in which the subjects caught the pennies, two at a toss, palm of the hand down. (2) That in which they caught three. The first series was of 7 days' duration; the second, 10 days'. The time for tossing was from 1. P.M. to 2 P.M. on Mondays and Wednesdays. Conditions were as nearly constant as possible, the same room being used throughout the experiment. In the case of the two-penny series, the subjects caught for 10 trials and then rested for 10. In the three-penny series two subjects caught at the same time, the third subject resting. In the first case, score was kept by the two unemployed subjects in turn; in the second case, by the one unemployed subject.

Certain conditions influencing accuracy were noted, among which are the following: Some parts of the room were more conducive to accurate catching than others, that nearest the window being the most favorable. The pennies could be caught with most accuracy if no objects were in front of the subject to distract his attention. The tossing, when carried on before a blank, light-colored wall, was most successful. An increase in confidence and in accuracy resulted when a window was opened to admit new air. An interruption, as that caused by another person entering the room, was followed by a corresponding fall in score. The subject, by counting to himself his successful tosses, was stimulated to a better score. The nervous feeling of haste as well as nervousness caused by outside matters of importance to the subjects (such as pressure of work) tended rather to increase than to diminish their scores.

Each subject discovered and followed his own methods of tossing. After finishing the two series, the subject who had followed the method of throwing his pennies high into the air was able to catch an additional penny (making four in all) with very little effort. The other subjects tried this continually and failed, their hands striking the floor before the fourth penny was reached. The quick shutting of the hand was an important factor. One subject was materially helped by thinking of the word "grab" previous to each trial. In some instances, the second penny would be caught and lost, the first and third being retained. Although occasionally a subject would catch all three successfully without knowing it, yet the tossing can not be said to have become automatic.

The progress in learning was unsteady. Yet in each case there

was a gradual advance, noticeable particularly in the beginning. A warming-up period was universally experienced by each subject at the beginning of each day's practise.

In the second series, a transfer test was tried with the left hand before and after the practise series. This showed a considerable increase in ability to catch with the left hand.

AMOUNT OF TRANSFER CATCHES

Subject	Before Test	After Test	Per Cent. Gain
1	3	14	466⅔
2	11	32	290+
3	1	29	2900
Total gain	15	75	500

Painting and the Learning Process: C. M. SAX.

Although art and science are widely separated, they may cooperate in art education. Prevailing methods are indirect, depending upon a never certain transfer of training. During the three years the average student spends at art school, his course is as follows: Casts and still life in charcoal; still life in color; anatomy and perspective as formal subjects; the figure in charcoal; some composition, and, finally, painting the head and figure in oils.

Results show little transfer; for example, compositions show little knowledge of anatomy or perspective. Charcoal and oils have few identical elements in substance or procedure; in fact, specific habits formed in mastering charcoal often act preclusively when the student attempts to paint. Students who can draw, but not paint; construct, but not compose, or are draughtsmen, but not colorists, and their opposites are in the overwhelming majority.

Experiments now under way on the learning process as applied to painting seem to show that (*a*) preparation in charcoal and still life is unnecessary in painting figures; (*b*) efficiency depends largely upon correct analysis; (*c*) muscular coordination plays a minor part; (*d*) a direct method and generalized idea of procedure are essential and (*e*) the control of attitude is most important.

The Optimal Distribution of Time, and the Relation of Length of Material to Time Taken for Learning: DARWIN OLIVER LYON.

This paper was divided into two parts, it being in reality a discussion of two distinct questions: (1) "The Distribution of Time in Relation to Economy in Learning and Retention"; and (2) "The Relation of Length of Material to Time Taken for Learning." Concerning the first of these, it was shown that in estimating *economy,* not only must we consider the *time spent,* but the degree of *retention* as well. It was shown that individuals differ greatly, and that

where one could learn a set of ten stanzas in less time by the continuous method (*i. e.*, doing the work in "one sitting"), another individual could lower his total time by dividing the time spent into several periods, *e. g.*, by spending 5 minutes per day. With but 3 exceptions retentiveness was decidedly better by the divided-time-method. This was notably the case with nonsense-syllables and poetry. The most general statement that can be made, taking all materials and methods of presentation into consideration, is that the most economical method is to distribute the readings over a rather lengthy period,—the intervals between the readings being in arithmetical proportion. For example, with one individual in memorizing a poem of 20 stanzas the highest retentiveness was obtained by distributing the readings as follows: 2 hours, 8 hours, 1 day, 2 days, 4 days, 8 days, 16 days, 32 days, etc. The practical bearing of the results obtained on education in general was then considered. The above individual found that the most economical method for keeping material once memorized from disappearing was to review the material whenever it started to "fade." Here also the intervals were found to be, roughly speaking, in arithmetical proportion. For similar reasons the student is advised to review his "lecture-notes" shortly after taking them, and if possible, to review them again the evening of the same day. Then the lapse of a week or two does not make nearly so much difference. When once he has forgotten so much that the various associations originally made have vanished, a considerable portion of the material is irretrievably lost.

2. *The Relation of the Length of Material to Time Taken for Learning.*—Tables were presented to show that the relation depended almost wholly upon the division of the time spent in learning, *i. e.*, the distribution of the time-intervals. In other words, the relation, or *ratio,* depends upon the method used in memorizing. Only three methods were considered: The "continuous" or "mass" method; the once-per-day method; and the once-per-week method. Up to a certain point, with some individuals, when digits were used as material, the time varied directly as the square of the number of digits, *when the continuous method was used.* By the once-per-day method, however, the time varied, roughly speaking, directly as the length of the material. It was shown that in order to get the best results the same subject should take all the various lengths of material used, and that it would be unfair to distribute the varying lengths among different subjects. As only one method can be tried at a time, an experiment of this nature must needs extend over a period of several years. In the case of *prose,* by the once-per-day method, 500 words were memorized in as few days as the 95-word passage. The time may therefore be said to vary directly as the

length of the passage. The same holds true for digits and nonsense syllables, but not to so great a degree; for the number of days needed for 200 nonsense-syllables was considerably greater than that needed for 20. By the "continuous" method, however, we observe that where the 100-word passage was memorized in 9 minutes, the 500-word passage took 52 minutes—nearly 6 times as much time being required, although the passage is only 5 times as long. This is much more strikingly shown when we examine the curve obtained for the digits. Here we see that although it took only 5 minutes to learn 24 digits, it took 2 hours and 34 minutes to learn 200—more than 31 times as long instead of 8. In short it is obvious that the once-per-day method is—to say nothing of giving a far superior retention—far more economical than the "continuous" method. This is especially so for material memorized by motor associations such as nonsense-syllables or digits.

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REVIEWS AND ABSTRACTS OF LITERATURE

The Essentials of Mental Measurement. WILLIAM BROWN. Cambridge: University Press. 1911. Pp. vi + 154.

The structure of Mr. Brown's book, as he himself points out, bears marks of its composite origin. The first three chapters of Part II., originally published in 1910 as a doctorate thesis, deal essentially with some experimental work with mental tests similar to the work of Winch and Burt, prefaced by a disquisition on the theory of correlation and a brief historical survey of the use made of the theory. To these chapters he has added a fourth sketching the possibilities of the use of the method of correlation by psychologists, and a Part I. treating of mental measurements in general. There are also four appendices, giving tables quoted from Fechner, Müller, and Urban, examples of the working out of single and multiple correlations, some regression curves in illustration of one of his earlier chapters, and a copious bibliography.

The result is an abstruse, slightly critical, mathematical treatise on the measurement of variables, rather than the expression of interest in things mental which might be investigated. With the exception of a concise statement of conclusions (pp. 126–27) the author leads one to forget any connection with psychologic functions, since the data supplying the basis for his mathematical elaboration might apparently have been drawn from any convenient source.

As it stands, Chapter I. of Part I. takes up Weber's law, Fechner's work with it, and three general interpretations of the law. Chapter II. sets out

to explain the three psychophysical methods. Clear in the beginning, the treatment becomes involved by the "slightly controversial tone" employed in the discussion of some other mathematicians' formulæ, and by the digressions necessary to set forth his own objections. For purposes of exposition these digressions might have been placed in footnotes rather than embodied in the text. Yet, condensed as they are, they assume considerable facility in statistical methods on the part of the reader, who, if insufficiently prepared, may refer to the more elementary treatments of Titchener, Pearson, Sheppard, Spearman, Urban, and others.

The Introduction to Part II. gives a concise "preliminary view of the method" of correlation, illustrated by examples and two figures. Chapter I. takes up the correlation coefficient, correlation ratio, probable errors, multiple correlation, six short-cut ways of determining correlation, spurious correlation, and the significance of the correlation coefficient. Much of this is beyond any but the "professed psychologist" prepared to undertake quantitative research, especially as the already sufficiently difficult accounts of Yule and others are here very much condensed. Emphasis is laid on the importance of not disregarding skew curves, and a relatively large space is devoted to a discussion of Pearson's criticisms of Spearman's ranking method. Chapter II. is a sketch of the use made of correlation by Wissler, Thorndike, Burt, Pearson, Elderton, and others, but is mainly taken up with Spearman's researches and the author's own refutation of some of Spearman's conclusions and formulæ. Particular notice is taken of the theory of both Spearman and Burt that there may exist a common, fundamental, mental function or group of functions as demonstrated by the hierarchical order of correlation coefficients. Chapter III. embodies the results of Mr. Brown's experiments with three groups of school-children and three of adults, about 260 subjects in all, in ten or eleven mental tests. These were undertaken in order to determine the extent of correlation between simple mental abilities and the relationship between them and general intellectual ability as measured by teachers' judgments, school marks, etc. It is interesting and unusual to find included the Müller-Lyer illusion, and a little disappointing not to find further work with some of Burt's original tests. Full tables are inserted giving results in each test, and correlation coefficients for the various groups separately. Some of the more general of the fifteen conclusions which close the chapter are:—that the Ebbinghaus *Combination* test is a good measure of intellectual ability, that "mechanical memory correlates fairly closely with intelligence," that "correlation between speed and accuracy of mental performance is slightly non-linear," and that "in homogeneous groups of subjects there is no positive evidence of the existence of one central factor." A brief final chapter works out a proof to emphasize the possibility of measuring correlation between complex variables, and for more than two variables.

The author expresses a hope that this book will prove of use to the educationist who has had "a real training in psychology." Such real training would have to be largely along the lines of quantitative research before this little volume could be appreciated. It will be valued by Eng-

lishmen, under the influence of Karl Pearson, and contributors to *Biometrika* rather more, perhaps, than by students and investigators in this country.

M. T. WHITLEY.

TEACHERS COLLEGE, COLUMBIA UNIVERSITY.

Outline of a Course in the Philosophy of Education. JOHN ANGUS MACVANNEL. New York: The Macmillan Company. 1912. Pp. 207.

This book is a revision and extension of a syllabus used in Teachers College, Columbia University. The leading topics are philosophy, persistent problems and presuppositions of education; the place of education, the individual and society, institutional factors, course of individual development, democracy and education, the school as a social institution, and the intellectual organization of the school. Many subdivisions of these main topics are given and at the close of each chapter is placed a considerable list of references to both ancient and modern books.

The author believes the most "fruitful study of education consists in treating it as an integral part of a wider philosophy of society," and he attempts to show the relation of the educational process to the facts of organic and social evolution. Education as a social institution is defined as "the method by which a particular generation endeavors to incorporate the vital elements of its civilization or culture into the life of the generation that succeeds it." Development is not merely "an unfolding from within, but also an enfolding from without the individual," and environment can not be considered separately. Heredity and environment "are in reality phases of the actual concrete working self." "The fundamental ethical need of men is self-realization." "Self-realization is a process in which the self (*a*) comes to be more completely defined, *i. e.*, individualized; (*b*) but defined through the membership in the larger unity." The functions of education "are (1) the liberation of the individual from himself and (2) the discovery of the individual to himself."

The book has many such aptly worded definitions and statements of truths in accordance with the best educational thought of the times, and those who are interested in correlating their ideas of philosophy and education after the manner of philosophers will appreciate the work highly. There is nothing, however, in the book to give direct aid to those who are studying educational problems in a scientific or practical way.

The ideals of every one who attempts to deal with educational problems are determined by his philosophy of life, whether he has ever formulated it or not. A conscious study of philosophy may be very valuable to an educator by leading him to form broader and truer ideals of life, but after such ideals have been formed the important thing is to interpret and apply them in a concrete form. Men may agree on a theoretical statement and differ radically as to subjects and methods to be used in applying the theory or even as to the results desired. For example, the religious ascetic of older times might, in learning to ignore the body and its desires, have claimed that he was performing the educational function of "the liberation of the individual from himself" and the New England puritan might

be justified in his frequent self-examination by the principle that the function of education is "the discovery of the individual to himself." Dr. MacVannel would undoubtedly protest emphatically against such interpretation of his statements and perhaps against a dozen other interpretations that might honestly be made by others.

It is not his purpose to make applications in this brief outline he has written and hence he is not to be criticized for not doing what he did not attempt. The question in the mind of the writer, however, remains, "Is there any real value in *any book* on the philosophy of education that concerns itself with the philosophy of the theories without indicating how it is purposed to interpret and apply these theories to concrete problems?" The statement that development is not merely "an unfolding from within but also an enfolding from without the individual" is very good, but it might be offered to the agriculturist with just as much propriety and significance as to the educator. Farmers and scientific students of agriculture consciously or unconsciously assume this truth and are trying to interpret and apply it in choosing seed, soil, fertilizers, etc. They would be disgusted with any one who asked them to study such fundamental assumptions under the name of "philosophy of agriculture" if there were no attempt made to point out the kind of problems to which such general definitions and general truths can be applied. Why should not the educator be equally pragmatic?

Dr. MacVannel has done the work he undertook to do well, but the writer wishes to raise the question among educators and philosophers as to whether it is possible for any philosopher as such to construct a philosophy of education that will be of any more real value to educators than would be a philosophy of farming to the farmer or a philosophy of manufacturing to the mill man or the philosophy of mining to the miner. Psychology is largely freed from philosophy. Why should not education as a science and art also be independent of it?

E. A. KIRKPATRICK.

FITCHBURG, MASS.

JOURNALS AND NEW BOOKS

THE PHILOSOPHICAL REVIEW. March, 1912. *Evolution* (pp. 137–151): FREDERICK J. E. WOODBRIDGE. – "Evolution is history; that antecedents and causes should consequently be historically construed; that evolution is pluralistic, . . . that man writes the history only of his own world; that, however, since he discovers his world to be a history, he may have a science of history or evolution which is universal; and this science indicates that evolution is progressive." *The Relation of Consciousness and Object in Sense-Perception* (pp. 152–173): EVANDER BRADLEY MCGILVARY. – A defense of an epistemological monism and realism with the view that consciousness is a unique and not further analyzable relation of "togetherness." The difficulties such as qualitative differences, how one

object may be in different consciousnesses, hallucinations, the time problem, color-blindness, and the "consciousness of consciousness," are examined. *Moral Experience* (pp. 174–188): LOUIS W. FLACCUS. – The term "moral experience" as used in current interpretations of ethics is, it is claimed, too vague. Its meaning as employed by biological, psychological, and autoteleological (including the Kantian type and current personal idealism and pragmatism) methods is unsatisfactory. The advantages are on the side of personal idealism and pragmatism. *Proceedings of the American Philosophical Association; the Eleventh Annual Meeting, Harvard University, December 27–29, 1911* (pp. 189–217). – Includes reports of committees with summaries of papers and discussions. *Reviews of Books* (pp. 218–238). B. Croce, *Lebendiges und Totes in Hegel's Philosophie:* FRANK TILLY. Th. Ruyssen, *Schopenhauer:* RADOSLAV A. TSANOFF. G. F. Barbour, *A Philosophical Study of Christian Ethics:* T. B KILPATRICK. A. W. Moore, *Pragmatism and its Critics:* THEODORE DE LAGUNA. *Notices of New Books. Summaries of Articles. Notes.*

REVUE DE MÉTAPHYSIQUE ET DE MORALE. March, 1912. *La philosophie des sciences historiques dans l'Allemagne contemporaine* (pp. 129–168): CH. ANDLER. – The work of the historian leads naturally to philosophical results. In particular, the conflict between descriptive and sociological history has placed the problem of the *a priori* which presides over the formation of historical concepts. *Translation solaire ou déformation du système sidéral?* (pp. 169–192): F. MARGUET. – The author suggests an astronomy based on the deformation of a tetrahedron formed by joining, say, the earth and three other planets, or the sun and two planets. *Devoir et durée* (pp. 193–206): J. WILBOIS. – The sketch of an ethics which shall mark out a science of morals, establish the moral imperative, and make the imperatives precise, or, at need, revise them. *La logique déductive* (Suite et fin) (pp. 207–231): A. PADOA. – The conclusion of the author's exposition of the symbolic language of Peano and the mathematical logicians. *Études critiques. Victor Brochard, philosophe et historien de la philosophie:* A. RIVAUD. *Questions practiques. Le Syndicalisme jaune:* F. CHALLAYE. *Supplément.*

Bowne, Borden Parker. Kant and Spencer. Boston: The Houghton Mifflin Company. 1912. Pp. xii + 440. $3.00.

Grünbaum, A. S. The Essentials of Morbid Pathology. London: Longmans and Company. Pp. xvi + 219. 7s. 6d.

Lones, T. E. Aristotle's Researches in Natural Science. London: West, Newman, and Company. Pp. viii + 274. 6s.

Rand, Benjamin. The Classical Psychologists. Selections illustrating psychology from Anaxagoras to Wundt. Boston: The Houghton Mifflin Company. 1912. Pp. xxi + 734. $3.50.

NOTES AND NEWS

HERR RUGE, privat docent at Heidelberg, is publishing, with the collaboration of Herr Windelband, a collection of systematic studies devoted to various branches of philosophy. The enterprise is unique in that the studies will consist of articles written by representatives of the principal contemporary schools of philosophy. The first volume, now in press, will contain: Wilhelm Windelband, "Die Prinzipien der Logik"; Josiah Royce, "Principles of Logic"; Louis Couturat, "Les Principes de la Logique"; Benedetto Croce, "Il Compito della Logica"; N. Losskij, "Die Umgestaltung des Bewusstseinsbegriffes in der modernen Erkenntnistheorie und ihre Bedeutung für die Logik." The general title of the work is "Encyclopädie der philosophischen Wissenschaft."

THE Aristotelian Society held meetings on June 3 and 5. At the former a symposium on "Purpose and Mechanism" was carried on by Professor W. R. Sorley, Mr. A. D. Lindsay, and Dr. Bernard Bosanquet. At the second meeting, Mr. W. E. Tanner read a paper on "Significance and Validity in Logic."

THE death is announced of Dr. H. de Struve, who from 1871 to 1903 was professor of philosophy at the University of Warsaw. A correspondent of *The Times* states that Dr. de Struve may be claimed as the founder of the present-day school of philosophy in Poland.—*Nature.*

AT a meeting of the British Academy, on June 5, the Rev. Hastings Rashdall read a paper on "The Metaphysic of Mr. Bradley." The paper dealt with Mr. Bradley as an idealist and with various problems as to the essence of reality.

A NEW critical edition of the works of Schopenhauer in fourteen volumes is to be undertaken by Paul Deussen. Two volumes, devoted to "Die Welt als Wille und Vorstellung," have already appeared.

THE inauguration of Dr. Anna McKeag, formerly head of the department of education in Wellesley College, as president of Wilson College, occurred on May 1.

PROFESSOR D'ARCY W. THOMPSON, professor of natural history at Dundee, has been appointed Herbert Spencer lecturer at Oxford for 1912.—*Science.*

AT the anniversary meeting of the Linnean Society, on May 24, Professor E. B. Poulton was elected president for the ensuing year.

DR. JOHN E. CLARK, instructor in history and philosophy in Boston University, has been appointed professor of education in that institution.

A NEW magazine, *Comment enseigner,* has just been launched in France. It is to be issued every three months.

DR. DANIEL STARCH has been promoted to the rank of assistant professor at the University of Wisconsin.